selection will not, in most cases, make much difference in the maintenance of such adjustment. Obviously this ground of objection to the theory of the cessation of selection opens up a much larger question than can here be dealt with, viz. the adjusting or eliminating value of the presence of selection. But if Prof. Weldon will read what I wrote last year in the *Contemporary Review*, during the Spencer-Weismann controversy, he will find that in this matter I am quite on the side of Mr. Bateson and himself. It has always been my endeavour to argue that the ultra-Darwinian school of Wallace and Weismann are pushing deductive speculation much too far in maintaining "The All-Sufficiency of Natural Selection." I shall never believe —any more than Darwin believed—that what I have called "selection value" is unlimited. But this is not incompatible with the belief that *in whatever degree* natural selection may have been instrumental in the construction of an adjustment, *in some degree* must its subsequent cessation tend to the degeneration of this adjustment, especially where complicity as distinguished from size is concerned, as stated in my last letter.

Summing up his objections to the doctrine of Panmixia, Prof. Weldon says they are two: "First, it is based on the assumption that selection, when acting on a species, must of necessity change the mean character of the species—an assumption incompatible with the maintenance of a species in a constant condition." This refers to the paragraph of his letter which, as already stated, I do not understand. The doctrine of Panmixia, as far as we are now concerned with it, does not refer to "species," but to specific characters, *i.e.* structures, organs, instincts, &c. Again, the doctrine, even with regard to specific characters, makes no "assumption" touching the *presence* of "selection acting on a species"—least of all that such *presence* will not maintain the species in a constant condition. On the contrary, the very essence of the doctrine is, that it is the *presence* of selection which *maintains* the constancy of a species (or specific character), and *therefore* that it is the *cessation* of selection which *upsets* the constancy by withdrawal of the maintaining influence. Hence, I do not understand Prof. Weldon's first objection. His second is, "that in the only case which has been experimentallv investigated, the condition said to result from a condition of Panmixia does not, in fact, occur." This one case, he explains, is:—"Mr. Galton has shown that civilised Englishmen are themselves in a condition of Panmixia, at least with respect to several characters, especially stature and the colour of the eyes. Now the mean stature of Englishmen is known to be increasing, and there is no evidence of the disappearance of coloured eyes." But, as regards stature, it can scarcely be maintained that there is not *some* cause at work to account for the increase; yet, unless this is maintained, the case is clearly irrelevant. Again, the colour of the eyes of our mixed population cannot have had more than thirty or forty generations wherein to be affected by Panmixia, and therefore the most ardent supporters of this doctrine would scarcely expect any result to be yet appreciable in the case of so pronounced a racial character. Surely a better "case" is the one which I have already given in the most ancient and the most rapidly-breeding of our domesticated animals. It was the facts observed in this "case" which first suggested to me the doctrine of Panmixia, and so led me to question the inherited effects of disuse. Similarly, a year later, Mr. Galton, in his "Theory of Heredity" (which anticipated by about ten years all the fundamental parts of Weismann's), wrote of Panmixia thus:—"A special cause may be assigned for the effects of disuse in causing hereditary atrophy of the disused parts. It has already been shown that all exceptionally developed organs tend to deteriorate; consequently those that are not protected by heredity will dwindle. The level of muscular efficiency in the wing of a strongly-flying bird is like the level of water in the leaky vessel of a Danaid, only secured to the race by constant effort, so to speak; let the effort be relaxed ever so little, and the level immediately falls. . . . That this is a universal tendency among races in a state of nature, is proved by the fact that existing races are only kept at their present level by the severe action of selection." GEORGE J. ROMANES.

Oxford, May 5.

P.S.—I gladly accept the verbal correction in Prof. Weldon's third paragraph.

Physiological Psychology and Psychophysics.

OWING to my bookseller's habit of forwarding NATURE in monthly batches, I have only just seen the remarks appended to my letter in the issue of March 15. I think that the terminological question is sufficiently important to warrant a reply to these.

(1) I do not, of course, "subsume" psychophysics to physiological psychology. The latter, I stated, is both wider and narrower than experimental psychology; and wider, because it includes the consideration of certain ("the most important") psychological problems—not of *all* such problems. (For this view of physiological psychology, *cf.* Wundt, "Physiological Psychology," fourth edition, I. p. 9.)

(2) Fechner, "the coiner of the word," defines psychophysics as "eine exacte Lehre von den functionellen oder Abhangigkeitsbeziehungen zwischen Körper und Seele, allgemeiner zwischen Körperlichen und geistigen, physischer und psychischer Welt." (*cf.* "Psychophysik," second edition, I. p. 8.) What my critic says on this head is, therefore, incorrect.

(3) In the most widespread and important school of experimental psychology existing to-day—that of Wundt—there *is* agreement upon definitions. And even if my critic's remarks were true, it would not follow that a number of wrongs made a right.

(4) I might, in my last letter, have adverted upon the term, *psycho-physiological.* I did not understand what it exactly meant. In NATURE of March 29, Prof. Ll. Morgan defines it (p. 504) as the equivalent of Fechner's internal psychophysics. (*op. cit.* p. 10). In this sense it is not wanted; the phrases "external" and "internal psychophysics" are in use. (It might, however, be used to signify that part of physiology which has a conscious correlate.)

(5) My critic triumphantly adduces "reaction-times" as a subject treated of in the University College course. That course, *i.e.* deals with *one* conscious element, and with *one* type (action) of *one* of the two modes of conscious combination (association: fusion is left out of account). Prof. Münsterberg (Preface to *Psychological Laboratory of Haward University*) speaks of "the error, which is so prevalent, that experimental psychology is confined to the study of sensations and simple reaction-times."

(6) I am sorry that Dr. Hill's name should have been mentioned. I should not think of offering any opinion upon his work. I know no more of it than do the other readers of NATURE. If he sees these remarks, I hope he will believe that my original criticism was meant to be quite impersonal.

(7) "By far the larger part of the really fruitful work [in psycho-physiology]" says my critic, ". . . . has been done in the investigation of the senses." If he means by psychophysiology what Prof. Ll. Morgan does, I must disagree with him. To substantiate either view would need an article. As he writes not as a working psychophysicist (else he would have been acquainted with Fechner's *Psychophysik*), I think that the *onus probandi* lies with him.

(8) As to Prof. Ll. Morgan's paper on "the scope of psychophysiology [= internal psycophysics]," I must plead guilty to finding the writer's eclecticism somewhat unintelligible, and his whole treatment a little general and superficial.

(9) A very interesting minor question is that of the relation of Wundt's physiology psychology to Fechner's internal psychophysics. (*cf.* Külpe, *Arch. f. Geschichte du Philosophie*, 1892, pp. 183-4.)

Cornell University, April 16. E. B. TITCHENER.

IT seems hardly profitable to carry on a discussion with Dr. Titchener at intervals of more than a month. I readily confess that through an error of memory, for it is a good while since I read the "Elemente der Psychophysik," I misrepresented Fechner's use of the term psychophysics. The fact, however, that he recognised an "outer psychophysics," and the further fact that, as he shows ("Elemente," i. p. 11), nearly the whole of his inquiry has to do with establishing the relation of external stimuli to psychic phenomena, show that the error I fell into was not altogether unnatural. Are not the inquiries of Weber, Fechner, and their successors still brought under the head of psychophysics by those who reject Fechner's peculiar "psycho-physical" interpretation of the results? And do not nine students out of ten, who are not themselves "working psycho-physicists," associate the term "psychophysics" with these important lines of inquiry? If so, I would contend that there is room for a reconsideration of the terminology of the subject. The retention of Fechner's "outer psychophysics" seems confusing if, as I understand Dr. Titchener to say, "psychophysics' has properly to do with the correlation

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between psychic processes and intra-organic and (I suppose) more especially, central nervous processes. With respect to the term "psycho-physiological," used by Dr. Hill in his syllabus, it may at least be said that it avoids the ambiguity of "psychophysics," as coined and defined by Fechner, while it is the direct descendant of the term "mental physiology," which is well fixed in British scientific literature.

I have only to repeat that Dr. Titchener's attempt to distinguish between the domains of physiological psychology and psychophysics seems to me far from adequate. It has about it, to my irreverent eyes, something of Wundt's own oracular obscurity.

In calling atttention to reaction-time experiments, I did not refer to *simple* reaction-times. I thought the various lines of experiment in which the processes of hesitation and selection, and so forth, are elucidated by measuring the intervals between sensory stimulus and muscular reaction might be brought under the head of reaction-time experiments. But not being in the privileged circle of working "psycho-physicists," I daresay I erred here too.

This little discussion will not be in vain if it wake up Dr. Titchener, or some other working psycho-physicist, to the obscurities that hang over their new field of research for the outside student. It seems to me that we want careful definitions of the respective scopes of the several departments of research which are either psychological or which bear directly on psychology, more especially experimental psychology as a whole, psychophysics, and physiological psychology. Neither Wundt's nor Dr. Titchener's definitions satisfy some of us who, on this benighted island, provokingly placed between two luminous continents, are doing our best to catch some of the rays which they are shooting forth in such abundance.

THE WRITER OF THE NOTE.

Some Oriental Beliefs about Bees and Wasps.

SINCE Baron Osten Sacken's letter appeared in NATURE (vol. xlix. p. 198), I have been taking an occasional survey in my small library of Oriental literature, to inform him of passages referring to the Bugonia-superstition. So far as I could find, the people of the far East seem not to have possessed any belief about oxen-born bees; however, *a propos* of this matter, I have come across several legends relating to some Hymenoptera, which I may group as follows:—

(1) *Fossores Story.*—Of all the insect stories of the far East, this may claim very high antiquity; it was first celebrated, more than two thousand years ago, by a verse in the Confucianist "Book of Poems," and is, to this date, preserved by a well-known metonymy "Ming-ling" (that is, the caterpillar), meaning the Foster-Child. This story, according to Yáng Hiung, a Chinese philosopher (53 B.C.–18 A.D.), was that "the Fossores, having no females, capture infant caterpillars from mulberry-trees, and address them a spell 'Mimic me, mimic me," whereby they are turned into the young Fossores." Indeed, the Japanese name of the Fossores is Jiga (that is, "Mimic-me"). Against this Teóu Hung-King, a Tauist sage (452-536 A.D.), has argued that these insects have had offsprings of their own, but used to deposit the eggs on bodies of other insects to provide them with food in future.

(2) *One-legged Wasp.*—In Li Shi-Chin's work, cited above, we read:—"This production of Ling-nán, resembling a wasp, small and black, has one leg united with the root of a tree; it can move but cannot escape." Also a One-legged Ant is mentioned. I would suggest that these insects were infested by the forms of Cordyceps, as is instanced in the stories of La Guêpe Vegétale.

(3) *Fungus-born Wasp.*—Twan Ching-Shih's "Miscellanies," book xvii., contains the following note:—"A poisonous and noctilucous Fungus of Ling-nán is, after rain, metamorphosed into a large black wasp with serrate mandibles more than three-tenths of an inch long. At night it tries to enter the ears and nostrils of a man, and hurts him in his heart."

(4) *Production of Amber from Bees.*—In the same work, book xi., is the following quotation from the "Record of Southern Savages":—"The Bees-with-Broken-Waists exist in the sands of Ning-chau, and come out when banks fall down; the natives make amber by applying fire to them." Obviously this erroneous inference was drawn from the presence in amber of some hymenopterous remains.

(5) *Diptera mistaken for Hymenoptera.*—Sie Tsâi-Kang, in his "Miscellanies of Five Phenoména" (Japanese edition, 1661, book ix., p. 43), narrates thus:—"In Chang-sha I saw honey-bees all without stings, so that, when trifled with upon the palms, they were quite harmless: having no difference from flies, that was strange!" No doubt he has seen some Eristalis, as is indicated by Baron Osten Sacken.

(6) *Horse-hair Wasp.*—Tazan Kan, a Japanese literatus (1748-1827), writes on this subject in his "Rambling Notes" (Tokio, 1890, p. 22):—"About 1817 a half-rotten trunk of *Celtis sinensis* gave birth to wasps, whose tails they could not withdraw from the tree, thus causing many to die. Having the tails cut with scissors the survivors gladly departed. One winter a man bought a heap of fuel comprising a half-rotten oak abounding with the similar wasps, several of which were strung on a horse-hair in the same manner as a rosary, there being altogether several dozens of such hairs. The author's informant took home a hair passing through three or four wasps, and folded it in paper; afterwards the hair became divided, and the insects bit through the paper: the informant's suggestion was—'probably these wasps had been transformed from horse-hairs tangled round the rotten wood.'" Several times I have seen in Japan this so-called "Babi-bō" (the Horse-hair Wasp), still an object of popular amazement: it is nothing but an ichneumon-fly, *Bracon penetrator*, whose ovipositor of unusual length has been the principal cause of such a superstition.

KUMAGUSU MINAKATA.

15, Blithfield Street, Kensington, W., April 30.

P.S.—In my letter on the "Constellations of the Far East" (NATURE, vol. xlviii. p. 542), I gave from Twan Ching-Shih's "Miscellanies" portions of the list of the objects of Indian fancy as to the resemblances of the constellations. Last March, my reverend friend, Atchârya Dharmanâga, then in Paris, kindly sent me an extract from Roshin Sennin's Lecture on the Constellations, recorded in Mahâsannipâta Sûtra. After comparison, I find that both quite agree except for a few variations, so that that Chinese author of "Miscellanies" seems to me to have extracted his list from the above-mentioned Indian authority.

K. M.

The Mass of the Earth.

I HAVE no intention of reopening a discussion on the advisability and necessity of carefully separating in our minds those two notions, the *weight* of a body and its *mass*, which to me (and to a great many others) are now so completely distinct. The subject has already been treated of in these pages. Hence, in reply to the letter signed "K." I shall be very brief. The *mass* of a body is simply the quantity of matter which it contains; its *weight* is the force with which the earth pulls the body towards the centre of the earth; this force varies slightly at different points on the earth's surface, varies very much both when the body is removed outwards from the earth or inwards towards its centre, and would be nothing at all at the centre; the *weight* would be practically nothing if the body were removed a few millions of miles away from the earth. But through all these changes, and through all mere changes of *place*, the *mass* of the body is perfectly unaltered. *Weight* is a mere contingent property of *mass*, logically and physically distinct from it; a more contingent property than *shape*; for, while a body must have some shape, it need have no weight. The terms *weight of the earth*, *weight of Jupiter*, *weight of the sun*, &c., are absolutely ridiculous. The mass of the earth is acted upon by no force whatsoever except the attraction of the sun and the disturbing attractions of the moon and the planets. The earth attracts itself with no force; it has no weight. So much for positive statement.

The notion that the earth has weight—the inability or neglect to distinguish the necessary property of constant *mass* from the contingent property of *weight*—has given rise to many absurdities. "If everything on the earth has weight, the whole earth has weight," is a fallacy of composition worthy of a mediæval dialectician. That the earth is a very *heavy* body (with, of course, an inveterate tendency to fall "down"), has supplied us with the assumption that it is supported on the back of an elephant, these two exceedingly *heavy* bodies being together supported on the back of a tortoise, and so on. To compare the *weight* of Jupiter with that of the sun, "imagine a gigantic balance with equal arms and equal pans; let the sun be placed in one pan; then, *in order to preserve the balance*, more than 1000 Jupiters must be placed in the other pan."

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